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THE PAST DECADE IN EXPERIMENTAL PSYCHOLOGY¹

By E. B. TITCHENER

I am to speak in this hour of the course and progress of experimental psychology during the past ten years. The psychological laboratory has, as you know, had but a short history; and the modern psychologist counts in decades, as the historian of human thought counts in centuries. It is, I hope, not out of place to remind you that even the century is an artificial unit; when we think of the philosophy of the eighteenth century we certainly include Locke in our list of writers, although Locke died in 1704; when we think of the psychology of the nineteenth century we certainly do not include Fechner, although Fechner came after Herbart. The century, none the less, serves a useful purpose, because a hundred years, three human generations, are as a rule sufficient for the testing of an idea, for its establishment or its final rejection. Of our own unit, the decade, we can say no more than that, in the precocious development of the later-born sciences, it is at least a period long enough to warrant an inventory or stock-taking, from which we may gauge the trend of interest in the immediate past, formulate problems for the present, and possibly infer the direction of inquiry in the immediate future. The ten years whose psychological activity I am to review have, however, a special claim upon the historian. Beginning in the nineteenth, they lead us at once across the line of secular division into the twentieth century; they embrace the culmination of the one and the first effort of the other; and I cannot resist the belief that all the sciences, old and young alike, approached this twentieth century with a certain self-conscious expectancy which, however difficult to appreciate to-day, will presently be seen to have exercised a marked influence upon the intellectual movement of the time. It is, then, if I read the signs aright, a period of unusual interest and of especial scientific significance that has formed the second decade in the life-history of Clark University.

Nevertheless, as I approach the topic of this lecture, what is

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uppermost in my mind is a sense of irreparable loss. When the cable brought the bare news, last February, that Ebbinghaus was dead, just a month after the celebration of his fifty-ninth birthday, the feeling that took precedence even of personal sorrow was the wonder what experimental psychology would do without him. You are all familiar with Ebbinghaus' work; to some of you, as to myself, his death has meant the loss of a friend: those who had not known him, but had looked forward to his promised address from this platform, have missed an experience that should have remained a lifelong memory. I shall not here attempt an eulogy: that is unnecessary. But I must remind you that Ebbinghaus' qualities were precisely those that, in its present stage of evolution, experimental psychology seems most to need. What characterized him was, first, an instinctive grasp of the scientific aspect of a problem,—scientific as distinct from philosophical, in all the protean meanings of that latter term; secondly, a perfect clarity of thought and of language, the expression of thought; and thirdly, an easy mastery of the facts. I say mastery; but the truth requires a stronger word. There was about Ebbinghaus a sort of masterfulness; he never did violence to the facts, but he marshalled them; he made them stand and deliver; he took from them, as of right, all that they contained; and with the tribute thus exacted he built up his theories and his system. This, I say, was the example that we needed, in a time when psychology still appears helplessly entangled with theory of knowledge; when the standard of scientific writing, so far as literary style is concerned, is deplorably low; when theory is impatient of fact, and the facts themselves are scattered and inco-ordinate. I believe, indeed, that Ebbinghaus' *Grundzüge*, already in its incomplete form a centre of widespread influence, was destined to a place of leadership; I have sometimes thought that, with allowance made for changed conditions, it might prove as important to experimental psychology even as Wundt's *Physiologische Psychologie* or Brentano's *Psychologie vom empirischen Standpunkte*. But Ebbinghaus is gone; and with his passing our science has sustained the most grievous loss that it has been called upon to bear since Delbœuf undertook that fatal journey to Munich thirteen years ago.

These brief remarks, inadequate as you must feel them to be, will at any rate in their spirit and intention command your assent. I pass now to another preliminary matter, upon which agreement is hardly to be expected. For agreement implies, in the first instance, a common point of view; and my own standpoint, which is that of pure science, or the desire for knowledge without regard to utility, is in all likelihood shared

only by a small minority of this audience. Moreover, agreement within the domain of pure science presupposes a certain measure of progress, a platform of assured results; and to that point, perhaps, experimental psychology has not yet attained. Nevertheless, while I anticipate that you will reject my conclusion, I trust that you will also remember the general attitude and point of view from which it is derived.

If, then, one were asked to sum up, in a sentence, the trend of psychology during the past ten years, one's reply would be: Psychology has leaned, very definitely, towards application. And if the questioner were thereupon to look for proof of this statement, he would find it confirmed not only by the range and variety of current practical work, but also and more particularly by the incursion into the field of practice of men whose training and previous interests might naturally have held them aloof. I shall not try to indicate the surprises that this movement towards application had in store for the theoretical psychologist; still less shall I try to set before you with any fullness the evidence for the strength and universality of the movement itself; I mention only a few typical facts. In 1903 Meumann opened the pages of his *Archiv. f. d. ges. Psychologie* to studies in applied psychology; and in 1905 Wundt alleges the preponderance of these studies as a principal reason for the foundation of the *Psychol. Studien*. In the same year 1903, Stern began the publication of his *Beiträge z. Psychol. d. Aussage*, which by 1907 had developed into the *Zeits. f. angewandte Psychol. und psychol. Sammelforschung*. Meinong writes in 1904 that experimental psychology, now brought into touch with the needs of practical life, is on the way to become a popular science; and expresses the hope that the contact of theory and practice may grow constantly closer. In 1906 Jung published the first volume of his *Diagnostische Assoziationsstudien*. In 1907 Meumann gave us two volumes of lectures on experimental pedagogics, with the promise of a system to follow. In 1908 Binet formally devotes his *Année psychologique* to the cause of practice; psychology is to be laid out and aligned with reference to practical and social questions. The present year has seen the publication, in English, of two popular works—Münsterberg's *Psychotherapy* and Watt's *Economy and Training of Memory*—which by diversity of aim and content as well as by form and style bear witness to the scope of application. I have mentioned such things as occurred to me: the more omissions you remark, the more securely will my thesis be established.

Now we have all heard it said—and said in connection with this practical tendency of recent work—that experimental psychology cannot hold its men; that its problems are not large enough to satisfy our intellectual demands; that the ex-

perimental psychologist will inevitably turn, sooner or later, to æsthetics or theory of knowledge, to physiology or general biology, to education or therapeutics. There is, in fact, some leaven of truth in that statement, and there is much untruth. Remember that no experimental science can hold its men beyond a certain term of life; remember that a large proportion of our students come late into the laboratory, and that they bring no habit of experimentation with them; and remember also the natural indolence of mankind, and how much easier it is to write an essay than to plan and carry through a series of experiments. But notice, on the other hand, that the experimental psychologist oftentimes has no choice allowed him; he may even hold his office under a foreign title; he is called to represent, in the economy of the university, a group of more or less closely related disciplines; and while no man to-day would claim for himself the title of physicist, it is not thought amiss that a philosopher or an educator should make a by-work of experimental psychology. All these things must be taken into account,—though I doubt if any one of them goes to the heart of the matter. The essential point is, surely, this: that many men, who are not by temperament psychologists, pass through the psychological laboratory on the way to their proper goal. Some are attracted by curiosity, by the mere charm of novelty; some desire in more serious mood to see and to understand. For all, psychology lies, convenient and accessible, at the cross-roads of human knowledge; and we who dwell there can only be grateful to those who for a season share our labors, while we hope, as soon as may be, to bid the casual visitor godspeed.

Under these conditions, the diversion into practical channels of energy which would otherwise have been expended in the service of the laboratory must be regarded as a definite loss to pure science; and it is from the standpoint of pure science that I am now speaking. You will reply, perhaps, that there are compensating advantages. For those who apply psychology testify, in the act, to the soundness and relative maturity of psychological ideas. And if we may judge by the experience of the older sciences, of physics and physiology, the trial of these ideas in practice will react upon the ideas themselves; application will discover new problems, which must be referred back for solution to the experimentalist. I recognize the advantages; but I do not think that they offset the loss. Every one, for instance, who has followed the history of science, knows that successful application is but a very imperfect measure of the validity of a theory; material improvement may go astonishingly far under the guidance of some scientific hypothesis which later generations

roundly pronounce erroneous, and which shows, indeed, but a glimmer of the later found truth. Pragmatism, as it was at first interpreted by its critics, and as it seems at first to have been intended by some, at least, of its propounders, could never pass muster with men of science, who see how well ideas may work upon how precarious a basis of fact. But, if we leave generalities aside, and consider the actual status of experimental psychology, we are still forced to the conclusion that this first argument overshoots the mark; it ranks as final achievement what is, for the most part, no more than tentative suggestion. And as for that other argument, of give and take, action and reaction between theory and practice, I confess that I am a little tired of it. Some day, if analogy may be trusted, it will hold; to-day it is but the expression of a pious hope. For application, if I read the documents fairly, has proceeded in two principal ways: it has borrowed and transformed some approved psychological method, or it has adopted and popularized some assured psychological result. I have nothing but respect for the men who by ingenuity and hard work make so much out of so little; but I cannot believe that either of those procedures will, by reaction, bring any considerable gain to science.

These, however, are merely counter-arguments, rebuttals; and it behooves a minority to be aggressive. Let me add, then, a positive statement. So far is experimental psychology from any general readiness to furnish ideas for application, that applied psychology has been obliged to think out ideas for itself; and so far is applied psychology from reliance upon the parent discipline, that some of its most widely used and most strongly emphasized ideas contravene established scientific principles. The notion of a quasi-mechanical dissociation, for example, or various modern forms of the doctrine of the subconscious,—these ideas are both foreign to the spirit and inadequate to the status of experimental psychology; they are stumbling-blocks in the path of scientific enquiry, obstacles that it will take time and labor to overcome. Not only, therefore, has the movement of the last few years, by its withdrawal of men and its substitution of practical for theoretical interests, brought with it a loss to science; but it has also, in the manner indicated, placed positive hindrances in the way of scientific advance.

That is the situation, as it appears from my point of view: others will see it differently. It is, in any case, a situation that must be accepted; and we have now to consider how, because or in spite of it, experimental psychology, the theoretical psychology of the laboratory, has fared during the past ten years.

I wish that I could proceed systematically. But, as we have already seen, psychology is not ripe for systematization. Let that be said emphatically:—and when you hear criticism of the claims advanced or the promises made by experimental psychology, remember, in all justice, that it has been said emphatically, and be at the pains to discover whether the claims and the promises come from psychology itself or from some less responsible enthusiasm. We must, in fact, proceed topically, though we shall naturally begin with the three fundamental topics of sensation, affection and attention.

In the sphere (1) of *sensation*, the most striking fact to record is the revival and extension of Fechnerian psychophysics. We have but to exceed our time-limit by one year, and we may start out with Martin and Müller's work upon the differential sensitivity (1899). Then follow Foucault's *Psychophysique* in 1901, Wundt's fifth edition in 1902, Lipps' *Grundriss* in 1903, Müller's *Gesichtspunkte und Tatsachen* in 1904, Aliotta's *La misura in psicologia sperimentale* and my own *Experimental Psychology* ii. in 1905, Lehmann's *Psychologische Methodik* in 1906, Wundt's sixth edition and Urban's *Statistical Methods* in 1908—to say nothing of a number of authors (Ament, Fröbes, Mosch, Holt, Laub and others) who have published single papers of considerable theoretical interest. Truly, there is hope for a generation which, in the midst of practical activities and despite the clamor for results, can find the time and the men to pursue in this persistent way the study of scientific method! And the outcome? The outcome is that, on the level of ordinary quantitative work, we now know enough to check our methods; we can prove our sums, as the children say; our figures have a definite scientific meaning and a general scientific setting. There will still be differences of opinion; there will still, no doubt, be controversies; force of habit would see to that, for some years to come, even if there were no questions of fact that remained unsettled. But it is a great deal, in science, to be sure of your ground, to have sifted out personal opinion and speculative guesswork from observed fact and genuine problem; and I think we may say, without undue optimism, that psychophysics has now reached that stage of self-knowledge, and has thus been lifted, with no fear of relapse, to a higher plane.

This, as I said, is the most obvious advance to record in the recent psychology of sensation. You could, indeed, hardly expect that progress should be as clearly marked on the side of quality, where the mass of established fact was already very large, and where—thanks to the co-operation of the physiologists—productive work has been continuous. If, however, I were called upon to mention a single noteworthy

event, I should select the re-issue of Hering's *Lehre vom Lichtsinn*, in which the veteran author has set himself, after the lapse of a generation, to theorize afresh the whole realm of visual sensation. Another gift from physiology is the third volume of Nagel's *Handbuch*, which will henceforth be to our students what Hermann's *Handbuch* has been to ourselves. And if we feel a natural regret that the old order should change and the authorities of our youth lapse into forgetfulness, let us at the same time recognize that the change does but emphasize the debt of psychology to its sister science: a debt, moreover, that must remain unpaid so long as physiology refuses the sole return that we can make, and physiologists decline instruction in psychology. For the rest, I may roughly indicate the progress of the decade by reminding you that we have to-day no satisfactory theory of sight or hearing, of taste or smell, of pressure or temperature; the generalizations that worked so well for science a few years ago, and that still do good service in the text-books, are in the minds of those who know breaking down under the stress of newly discovered facts; and the passing of established views is only one aspect of a process which, looked at from the positive side, leads to reconstruction. But again, if I must be concrete, I can choose a single instance: the revival during the last few years—in the hands of Meumann, Becher, Murray—of experimental interest in the organic sensations. When we remember the importance of organic sensation in the affective life, its importance as the vehicle of sensory judgments in psychophysical work, the part it plays in the mechanism of memory and recognition, in the motives to action, in the primary perception of self, in many of the complex formations that go by the general name of thought: when we remember the systematic questions that hinge upon it,—the question of the elementariness of pleasantness-unpleasantness, of the relative range of sensation and image, of what is called affective memory, and so on; we can hardly fail to see that these modest beginnings promise to fill up a great and painful gap in our psychological knowledge. It is too early to ask for results. It is something if we realize, in a fairly definite way, the difficulties of method that the experimenter must overcome before results are obtained; or rather,—it is everything that the experimental study of organic sensation has actually been attempted.

I undertook to speak of progress, not of problems. Yet I do not like to miss this opportunity of public reference to a problem which, although it has already been approached here and there, in a general way, seems to me just now to call pressingly for detailed treatment: I mean, the problem of sensible dura-

tion. In our analytical study of complex formations,—perceptions, perceptual groups, and especially total consciousnesses,—we must, I am convinced, make greater use of the temporal attributes of sensations. Is there any one who has not, time and again, been puzzled and surprised that experiences which felt so differently should come out so similarly in analytical terms? Well, experimental psychology, in its natural concern for intensity and quality, has unduly neglected the other aspects of sensation, and among them this aspect of duration. Wundt has set us a good example; he has employed duration, as an instrument of analysis, in his recent discussions of feeling and emotion; and I venture to suggest that the example is one to be followed in all departments of laboratory work.

This mention of Wundt takes us easily to our second introductory topic, the topic (2) of *affection*. Wundt, as you will remember, published in 1896 the first draught of his tridimensional theory, and in 1900 his *Bemerkungen zur Theorie der Gefühle*; Stumpf published in 1906 his paper *Ueber Gefühlsempfindungen*. These dates are significant: for they imply that the experimental psychology of feeling, which begins with Féré in 1887 and Lehmann in 1892, attains its full development in the decennium now under review. The threshold year 1899 is important here, by reason of Lehmann and Angell and Thompson, as we saw it to be important for psychophysics. Then follow, in quick succession, the experimental studies of Zoneff and Meumann, Brahn, Titchener, Gent, Bonsor, MacDougall, Orth, Boggs, Gordon, Störing, Kelchner, Hayes, Urban, Johnston, Keith, Shepard, Alechsieff, Salow, Kaestner, Nakashima,—studies that differ in method and in result as they differ also in scientific value, but that are all alike aimed directly upon an experimental control of the affective reaction. How near we have come to a stable psychology of feeling, I confess that I do not know; the smoke of controversy lingers, and it is impossible to get a clear view of the field of battle. Perhaps, for a little while, we shall none of us know; perhaps, we must await some new strategic movement to reveal the strength or the weakness of the various positions. I read, only the other day, that the attempt to build up feelings out of sensations has now been completely abandoned,—and I remembered, with some amusement, how many psychologists are still at work upon that task. No! in all such statements the wish is father to the thought, and the thought is but the wish become dogmatic. A more cautious estimate might, I think, venture upon three propositions, but could hardly go beyond them: first, that we have transcended, for good and all, that pseudo-Darwinism which, running directly counter to the intention and the accomplishment of Darwin's great work,

offers a facile teleology in place of scientific explanation, and deems the affective problem solved when it has written 'useful' for 'pleasant' and 'harmful' for 'unpleasant'; secondly, that our working hypotheses are adequate, intensively and extensively, in range and in detail, to serve as guides to experiment; and, thirdly, that the investigations so far published, inconclusive as you may judge them to be, nevertheless prove that experiment is possible. And this, you must remember, is the sole source of anxiety in an experimental discipline,—whether, when a question has been asked, the method of experiment can furnish a valid answer. So there is always, at the outset, a certain rush and hurry of research, and a certain immaturity in publication; but, the main point settled, and experiment shown to be feasible, the problem is put upon the regular waiting list, to be taken up at leisure. That is, as I see things, the present status of affective psychology. In the meantime, Wundt has made his tridimensional theory the basis of a psychology of language; and Stumpf has made his theory of a centrally excited accessory sensation the basis of a genetic psychology of the tonal and musical feelings: and while the two theories are incompatible, both of them appear to work.

I pass (3) to the psychology of *attention*, which I take to be in yet another characteristic stage of development, less advanced than the psychology of sensation, but more advanced than that of affection. Historically, such a state of affairs is but natural: for experimental work upon attention was begun by Wundt in the sixties, while the experimental study of affection dates only from the late eighties of the last century. And the bare fact that it thus occurs to one to measure progress by lapse of time is significant and encouraging, since time means nothing for science until methods have been thought out and men are trained to apply them. Nevertheless, I must not give you the idea that our knowledge of attention has increased continuously, little bit by bit, as the years have passed; that there is any historical analogy of that kind between sensation and attention. On the contrary, the doctrine of attention has shown very markedly the characteristics that certain psychologists ascribe to attention itself: it has fluctuated, risen and fallen again; wave of interest has alternated with period of indifference. Ebbinghaus wrote, in 1902, that attention is a real perplexity in psychology: "die Aufmerksamkeit ist eine rechte Verlegenheit der Psychologie;" and I think that he himself felt what he wrote; his treatment of attention is a little perfunctory, as if the composing of those sections had been a disagreeable duty. The same sentence stands in 1905; and, indeed, the whole discussion has remained practically unchanged

from the first to the second edition of the *Grundzüge*. Yet I believe that, at this latter date, one of our attention waves was gathering to a head. At any rate, the next three years saw the publication of as many systematic treatises: Pillsbury's book appeared, in French and English, in 1906 and 1908, and in 1907 came Dürr's *Die Lehre von der Aufmerksamkeit* and Roerich's *L'attention spontanée et volontaire*. It is true that the French and German authors are largely concerned with practical issues; but they deal with attention from the side of theory also; and Pillsbury's work, the most comprehensive of the three, devotes only a few pages to educational matters. Now, these men must have found something to say: and while their teaching is, in good part, inferential, even speculative, it rests, none the less, upon a broad basis of actual observation. There are two things, in particular, that to my mind attest the progress of the past decade. The phenomena that we have been wont to refer to as the fluctuations of attention are now shown, pretty definitely, to be peripherally conditioned; I, at least, am unable to put any other interpretation upon the facts; and the psychology of attention thus loses a chapter which has always been a fruitful source of confused thinking. For there are two main uses, in current investigation, of the term attention. In the practical studies of economy of learning, and the like, attention has an energetic meaning; we hear of energy of attention, distribution of attention, quickening of attention, recovery of attention, and so forth. All these phrases are readily intelligible, since they simply give precision to the usage of everyday life; and they are also sufficient for the writers' purpose, since they designate, unequivocally, certain well-marked events or phases of the process of learning. At the same time, it must be remembered that they are employed, casually and by the way, in the course of inquiries which are not themselves directed upon an analysis of attention. The point is important, if it is a little obvious, and I pause a moment to illustrate it. In work upon the affective processes, we often refer a judgment roundly to 'association;' in work upon association itself, we often record an 'attitude' of perplexity or doubt or hesitation as a link in the chain of interconnected processes: the abstract terms 'association' and 'attitude' meet the conditions of the inquiry, and no one can quarrel with their use. But no one, either, can doubt that they presuppose a psychology; they imply a foregone psychological analysis: and what holds of them holds similarly of the term 'attention.' Now, when we turn to the second, analytical meaning of attention, we find that our concern is with the clearness or vividness of conscious contents; energetics drops out of sight. But you will see that the notion of a fluctuation of attention, of apperceptive waves, suggests the ebb and flow of

energy, so that we are constantly tempted, even when engaged upon a purely analytical task, to take attention for granted, to employ it as *datum* in the very context which we have arranged for it as problem. It is in this way that confusion has arisen, and it is on this account that we may rejoice to refer to sensation phenomena that, ever since the days of N. Lange, have been attributed to the attention.

I said that there were two things that bore witness to the progress of the decade; and the second, which I can but briefly mention, is the revival, with good promise of success, of the attempt to measure the attention, to give quantitative expression to distinguishable degrees of clearness. It is still too early to discuss this work in any summary way. The impulse to it comes, most appropriately, from the Leipsic laboratory, and Wirth's further researches will not only be awaited with eager interest, but will also, as we may hope, arouse activity in other quarters.

So much, then, for sensation, affection and attention. Our topical survey will bring us next, I imagine, to that mixed medley of formations which is included under the general term (4) *perception*. It is altogether impossible to review, in the time allotted to me, the work accomplished in this field of vast range and uncertain limits: I have tried, as I was in honor bound to try, and I have given up. You must be content with a purely subjective and arbitrary selection. I found, as a matter of fact, after refreshing my memory of some hundred experimental papers, that I had been most impressed by the work of Benussi on optical illusions, on the perception of time, on the comparison of spatial distances and temporal intervals; it seems to me that Benussi's treatment of the apprehension of form, his notion of the inadequacy of idea, his doctrine of accental figure, and so on,—that all this is of positive value in itself, and will bring forth fruit in the future; and my judgment has remained the same, as study followed study, although I am by no means able to accept Benussi's interpretations. The Austrian school—however widely we may differ from them on systematic questions—have, indeed, made notable contributions to this chapter of psychology. For the rest, these ten years appear to have hardened and emphasized the special characteristics of American work upon the perceptual problem. Perception, in the cis-Atlantic laboratories, has been given a biological setting and perspective, and the theories of perception have been motor theories. I do not think that any good has come from the intermixture of biology; on the contrary, there has been a tendency to substitute final for efficient causes, and general considerations for exact psychophysical determination.

I am also heretical enough to think that our current motor theories are both premature and one-sided: premature because they far outrun our knowledge of the motor mechanism, and one-sided because they directly attach the motor to the receptive organs, and forget the disjunctive office of the cortex. But I do not, on this account, underestimate the achievement of Dodge and Münsterberg and Judd.

I pass from perception to the cognate subjects of (5) *recognition, memory and association*. Ebbinghaus, as I need not remind you, published his experimental study of memory in 1885; and there was no lack of experimental papers in the nineties. But consider our initial year, 1900. That year saw the issue of Müller and Pilzecker's *Exp. Beitr. z. Lehre vom Gedächtniss*; of Laura Steffens' work *Ueber die motorische Einstellung*, and Lottie Steffens' *Exp. Beitr. zur Lehre vom oekonomischen Lernen*; of Kemsies' and Netschajeff's articles on the memory of children; of Smith's *Rhythmus und Arbeit*. Our decennium began well; and it ends as it began; for it was only the other day that we received the second and concluding portion of Wreschner's elaborate memoir *Die Reproduktion und Assoziation von Vorstellungen*. In truth, this investigation of memory and association illustrates and accords with the tendency of the time. Ebbinghaus himself conceived his problem rather in a practical or psychophysical than in a psychological spirit; and, in the light of what I have already said, you will find it natural that his example should be followed,—all the more natural, indeed, since in this particular field psychophysics and practice are closely and obviously related. There are, however, in reality, three distinct problems. We may aim at a psychology of memory and recognition and association; that is, we may seek to record our experience, to trace introspectively the arrangement and course of consciousness as we remember or recognize or associate. We may aim, secondly, at a psychophysics; we may trace and measure the action of the reproductive and perseverative tendencies, evaluate the parts played by the reproductive and the recognitive factors, work out formulas of the same kind as that first formula of Ebbinghaus which represents retention as a function of time elapsed. Or we may aim, thirdly, at an applied psychology; we may lay down rules for the training and the economical use of memory. There can, now, be no doubt that the two latter aims have, in the period which I am reviewing, taken precedence of the first. But psychology has by no means been neglected: Cordes' analytical study of association appeared in 1901; Whipple's analysis of the memory image and the process of judgment in discrimination, in 1901-2; Gamble and Calkins' experiments

upon recognition and comparison in 1903; while Kuhlmann, who worked for the most part in the Clark University laboratory, has published in 1906, 1907 and 1909 a series of articles devoted especially to the introspective characterization of the memory consciousness. If to the results of these enquiries we add the indirect results of the psychophysical experiments—and indirect or secondary results have always been of great importance in experimental psychology—we have a considerable body of knowledge, even though it is not as yet either organized or complete.

I come next (6) to *action*. The reaction experiment we have had always with us, ever since the psychological laboratory came into being; it is an experiment that has been sorely abused, in word and in deed; yet so many are its possibilities, that interest in it has never flagged; and we are now beginning to see that it furnishes an invaluable instrument of psychological analysis. The decade opens with a number of technical papers: W. G. Smith in 1900 and 1903, and Judd and his collaborators in the Yale laboratory in 1905, have sought to determine the nature of the reaction movement; and Alechsieff in 1900, and Bergemann in 1905, have essayed new methods of quantitative evaluation. But this, the technical or psychophysical aspect of the experiment, has been overshadowed by the psychological. In 1905, Ach published a book entitled *Ueber die Willenstätigkeit und das Denken*,—a book which reports and discusses the employment of what the author terms, a little redundantly, the 'method of systematic experimental introspection' in a series of experiments upon simple and compound reactions. He could, indeed, hardly have chosen a more promising field. For although Kuelpe said as long ago as 1893 that "reactions are nothing else than exact types of . . . voluntary action, . . . so that their mere duration is but a small part of their psychological significance," and although Wundt has repeatedly endorsed this statement, no one before Ach had made any serious attempt to build up a psychology of volition and action upon the introspective data which the reaction experiment affords. In brief outline, Ach distinguishes a fore, mid and after period of reaction: the fore period extends from signal to stimulus, the mid or principal period from stimulus to reactive movement; the after period is a time of indefinite duration, but certainly lasting several minutes, which follows immediately upon the conclusion of the experiment. The method of systematic experimental introspection then requires that the events of the fore and mid periods be introspectively examined, as a whole, during the persistence of the perseverative tendencies in the after period. I shall have to refer, in

my second lecture,¹ to the results of Ach's investigations; it is enough now to say that those results have thrown a flood of light upon the action consciousness, and that they are happily confirmed by the outcome of other experiments, especially by the work of Watt, undertaken independently and for a different purpose. Hitherto, the chapter in our text-books devoted to the psychology of action has been a medley of evolutionary biology, physiology, and vague psychological generalization; henceforth, it will contain facts of psychological observation.

I wish that I could say as much (7) for *imagination*. But indeed, whether we begin with the elementary process, the image, or whether we go to the other extreme, and regard imagination as the general name for a group of typical formations, as a concept co-ordinate with memory, we must surely say that experimental psychology is to-day hardly over the threshold of the subject. My own belief is that we must start out with the image, and determine, for instance, whether image quality is co-extensive with sensation quality, and whether imaginal difference is adequate to sensible discrimination. More especially must we make a serious effort to resolve the paradox of the image: that it is so readily confusable with sensation, and yet so easily and certainly distinguished. Much may be hoped, I think, from the current studies of imagery, and from the incidental results of work upon thought and voluntary action. Thus we have recently found, in my own laboratory, that on one important point the popular psychology of memory and imagination inverts the facts: the image is available for memory, not because or in so far as it is stable and permanent, but precisely because it is instable and breaks down; because it admits of fusion, telescoping, substitution, and thus enables us to carry a great deal of experience in condensed and abbreviated form; while the imaginative mind, at any rate on the average level, is not a mind whose images change kaleidoscopically, producing by their instability new and still newer mental combinations, but is, on the contrary, a mind equipped with an almost photographically persistent imagery, which may be contemplated and arranged at leisure. This is natural enough, when you have discovered it; but it is hardly what you would have argued *a priori*. I might cite a number of similar results: but the facts stand in isolation; in the main, imagination is virgin territory, and awaits as it invites the pioneer.

Of the more complex (8) *affective formations* we can say but little until we have an assured psychology of feeling. I

¹ "The Psychology of Thought." This Lecture is not here reprinted, as the author has recently published a more extended discussion of the subject.

remarked, earlier in this lecture, that Wundt has made significant use, in his theory of emotion, of the temporal aspect of mental processes; and I think that the future experimenter will do well to take the hint. But we cannot analyze emotion and sentiment, with any prospect of final success, until we are agreed upon the nature and number of the affective qualities.

It is a relief to turn from these topics, of imagination and emotion, to the problem (9) of *thought*. We have found various things that may with some truth be called 'characteristic' of the experimental psychology of the last ten years: the revival and extension of psychophysics, the focalizing of the affective problem, the emergence of a tangible psychology of attention, the establishment of the laws of memory. Nevertheless, if I were asked to name a single line of investigation that, more than any other, has characterized the decennium, I should not hesitate to select the experimental studies of the thought-processes, most of which we owe to the Würzburg laboratory. Not that these researches have been confined to Germany: on the contrary, Binet in France, Woodworth in the United States, Bovet in Switzerland, as well as Marbe and his successors at Würzburg, have all attacked the same problem; though it is true that the German work has been the most thorough and the most persistent. Here is a new departure in experimental psychology; concept and judgment and inference, the last refuge of the rational psychologist, have been ranged alongside of sensation and association, introspectively analyzed and made subject to the chronoscope. I shall endeavor, in my next Lecture, to give you some idea of methods and results, and to point out the most promising paths of future enquiry.

So I finish the principal part of my review. If I have omitted anything of consequence, or if I have seemed to do injustice to any department of work, I must ask for pardon and correction; I have spoken with the utmost possible brevity. It remains, now, to say something of the extensions of the experimental method beyond the limits within which the present discussion has moved. What of individual psychology; of the psychology of the minor abnormalities—sleep, dreaming, hypnosis; of experimental æsthetics? What, last but not least, of comparative psychology?

I have time only for a word or two. Individual psychology, which was first systematized by Stern in 1900, is, in its modern form, one of the chief witnesses to the value of experiment. It furnishes the key to many, otherwise inexplicable differences of result, and it promises to allay many of the standing controversies of the text-books; there can be no

doubt that it will play a part of steadily increasing importance in the immediate future. The psychology of the abnormal seems, on the contrary, if I may venture an opinion, to have failed somewhat of its theoretical promise; recent interest has turned toward application. Vogt's method of 'direct psychological experiment in the state of hypnosis' has not helped us, as some had hoped it would, to an affective analysis; and although Martin and Ach have brought hypnosis into the laboratory, it does not appear probable that their example will be generally followed. Experimental æsthetics has shared in the recent horizontal expansion of psychology at large. A review of the work done upon colors and spatial forms, which appeared in the sixth volume of the *Année psychologique* (1900), mentions no more than five investigators; and there is an interval of more than twenty years between the first and the second, between Fechner and Witmer. But when Kuelpe came to prepare a similar review for the 1906 Congress of Experimental Psychology, he found a wide extension of subject-matter, a whole armory of methods, and a very considerable body of results. No doubt, the time is still distant, if indeed it is fated to arrive at all, when experimental æsthetics shall bear to a *System der Aesthetik* the relation that experimental now sustains to systematic psychology. In the meanwhile, it is satisfactory to know that, in this as in other directions, the American laboratories are doing their full share.

Again, however, there is something still more characteristic to follow: I mean, of course, the rapid growth of an experimental psychology of the lower organisms, a growth that is evidenced by the issue of books, the manifold publication of technical studies, and the invasion of our laboratories by various unwonted forms of animal life. The present status of comparative psychology has already been set forth by an authority far more competent than myself; and I desire now simply to give expression to my personal feelings, as the director of a laboratory primarily intended for the investigation of human consciousness. Personally, then, I welcome the animals, both for my own sake and for theirs. For my own, because I think that the comparative work exerts a wholesome influence upon the humanist. It is difficult to lay one's finger upon any definite point of indebtedness; but I am sure that many of my ideas have been quickened and clarified by acquaintance with the problems and methods of comparative research. And for the sake of the animals: because a comparative psychology can be built up only by men who have had their training in human psychology. Of that fact there can be no manner of doubt. The zoölogist may, and probably does, excel the psychologist in his mastery of biologi-

cal technique. But psychology also has its technique: psychology has its attitude, its point of view, its permissible and its impermissible questions, its methodological equipment, a technique that it takes time to acquire; and if you lack it, you may observe animal behavior with all the patience and all the accuracy conceivable, but you will not attain to a comparative psychology. Until such time, therefore, as experimental psychology forms a recognized part of every man's general biological training, training in physiology and zoölogy and the sciences of life at large,—until such time, the place for the animals who are to reveal the range and character of their mental endowment is the psychological laboratory.

I am almost at an end; and I have given you, I am afraid, little more than a string of names and a few general remarks; though, after all, one cannot do much in an hour. There is, now, one further point that I desire to discuss before I close. I have taken it for granted, throughout this lecture, that the primary aim of psychology is the analysis of mind. Yet a great deal has been written, of late years, against psychological analysis. Consciousness, we are told in effect, is a living continuum; but the analyst kills in order to make his dissection; and, after killing, he is unable to restore the life that he has taken, to show consciousness in its original integrity. This argument, if it were taken seriously, would apply to biology as well as to psychology, and would banish the muscle-nerve preparation and the microtome from the biological laboratory. But, indeed, it rests only upon misunderstanding,—a misunderstanding due in part to temperamental reaction, in part to the pressure of history and tradition. When the physiologist describes a tissue as 'composed' of muscle fibres or nerve cells, nobody takes him to mean that the fibres and cells existed first, in isolation, and that they were presently brought together, by some law of organic growth, to constitute the tissue. What grew was the tissue itself, which the physiologist now finds, in his *post mortem* examination, to consist of the cells or the fibres. Nevertheless, the analytical psychologist is supposed to generate his mind by allowing sensations to fuse and ideas to colligate, precisely as the physiologist might be supposed to generate a muscle by allowing the fibres to 'constitute.' In reality, to charge the analytical psychologist with deriving mind from the interconnection of the mental elements—and how often and how recklessly has not that charge been made!—is sheerly to misunderstand the purpose of analysis in the hands of those who use it.

The scientific legitimacy of the analytical attitude is, then, beyond dispute. Whether the results of analysis, in the sphere

of mind, are of 'value' is another question, and a question whose answer depends upon what one is disposed to consider valuable. What is psychology 'for?' If the object of the psychologist is to know mind, to understand mind, then it seems to me—in view of the overwhelming complexity of mind in the concrete—that his only course is to pull mind to pieces, and to scrutinize the fragments as minutely as possible and from all possible points of view. His results, in synthetic reconstruction, give him the same sort of intelligent grip upon mind that the analytical results of the physiologist give him upon the living body. To approach the study of mind without analysis would, in fact, be nothing less than ridiculous; and, in fact, no one does it. The most ardent advocate of mental integrity can follow up only one mental aspect or one mental function at a time.

I conclude that analysis is not only valuable, but indispensable to psychology; and I contend that many of the current arguments urged against psychological 'atomism' betray a woeful misunderstanding of scientific methods, and that much of the current depreciation of analytical results betrays a like misunderstanding of the aim of scientific psychology. But, in saying these things, I am only repeating what has been more aptly said by Ebbinghaus. And as I began this lecture by deploing the loss which Ebbinghaus' untimely death has entailed upon our science, so I cannot end it better than by reference to the admirable sanity which marks his treatment of the wider issues of psychology in the first volume of the *Grundzüge*.